RESEARCH MANAGEMENT

A PUBLICATION OF THE
INDUSTRIAL RESEARCH INSTITUTE

AUTUMN 1958
Volume I, Number 3

INTERSCIENCE PUBLISHERS • NEW YORK • LONDON

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SUBSCRIPTIONS: Price per volume: $7.50; to IRI members, $6.50. Foreign postage, $0.50 per volume. IRI subscriptions should be placed through the Industrial Research Institute, Inc., 100 Park Avenue, New York 17, New York. All other subscriptions should be sent to Interscience Publishers, Inc., 250 Fifth Avenue, New York 1, New York.

MANUSCRIPTS should be submitted to the Editor, Charles M. Burrill, RCA Laboratories, Princeton, New Jersey.

ABOUT THIS ISSUE

In this issue we add to our publication of papers presented at the semiannual meetings of the Industrial Research Institute a brief report of a statistical survey of patent policies and practices and two contributed papers.

We mention first the three papers which complete our selections from the program of the Spring Meeting of the Institute at Colorado Springs last May. Two of these papers are case histories of noteworthy research: one by Dr. Frank K. Schoenfeld, Vice President–Research, The B. F. Goodrich Company, on the development of a new class of synthetic hydrophilic colloids which they have named Carbopols; the other by Dr. S. D. Stookey, Manager, Fundamental Chemical Research, Corning Glass Works, on the development of Pyroceram, a new class of ceramics made from glass. We believe the reader will note with interest, in these research success stories, a common core of basic research procedures, and also specific problems and techniques that are unique in each one.

Dr. Schoenfeld is a chemist; he received his Ph.D. from Western Reserve University in 1927 and has been with Goodrich ever since. Dr. Stookey received his Ph.D. in Physical Chemistry from Massachusetts Institute of Technology in 1940 and thereupon joined Corning.

The third Colorado Springs paper was originally entitled "Research on Research Management." It is a case history of the developing organization for research in an expanding large corporation. Its author is Dr. Raymond W. McNamee, Manager of Research Administration, Union Carbide Corporation. Dr. McNamee, like our other authors of this issue, has found his career with a single company, having been with the Union Carbide

family since receiving his Ph.D. in Chemistry from Northwestern University in 1933.

Clyde L. Blohm, Manager of Research for the Fluor Corporation Ltd., who is Chairman of the Vital Statistics Committee of the Industrial Research Institute, prepared for *Research Management* the summary of that Committee's recent survey of patent policies and practices that we include in this issue. The membership of the Vital Statistics Committee that organized and conducted this survey included, besides Mr. Blohm, research executives from Celanese Corporation of America, International Business Machines Corporation, Lane-Wells Company, National Dairy Products Corporation, Owens-Corning Fiberglas Corporation, and Westinghouse Electric Corporation.

Our first contributed paper is by E. Duer Reeves, a past president of the Industrial Research Institute, who discusses the procedure followed in the Esso Research and Engineering Company in preparing the annual research budget. As Executive Vice President and Director of that Company, with 28 years experience with Esso since graduating from Williams College, he writes with insight on this subject.

All the aforementioned papers are by authors who are managing major research activities. Our second contributed paper, which concludes this Autumn Issue, is by a sociologist and consultant in management who is associated with the Esso Standard Oil Company in the field of organization research. Dr. Herbert A. Shepard gives us a social scientist's view of an important problem of research administration, how to avoid making poor managers out of good research workers. Dr. Shepard came from Canada, where he attended McMaster University and the University of Toronto. He came to the United States in 1948 and obtained his doctorate in Industrial Economics at Massachusetts Institute of Technology in 1950.

DEVELOPMENT OF THE RESEARCH BUDGET

E. D. REEVES

Esso Research and Engineering Company, Linden, New Jersey

An area of research management that receives far more attention and is more important to research people than most of them realize is the research budget. To begin with, why do we think we need a research budget? By this I do not mean to imply that we could get along without one because research people many years ago got over the notion that a budget was just a nuisance. Research budgets are, today, part and parcel of our research effort itself. Our concern now in formulating research budgets is with what they can contribute to the over-all effectiveness of our research effort. In order to explain what I mean by this, I would like to discuss just what a research organization is today and just what it is we are trying to accomplish through budgeting.

Modern industrial research is carried out by specific groups organized to discharge this function effectively. While there are, naturally, all sizes and kinds of research organizations, they are, in basic organization, beginning to follow somewhat similar patterns as research people work to develop the most effective type of organization. Our preoccupation with the best type of organization to carry out industrial research has been made necessary by the growing effort in this field and the highly competitive situation that has been generated. Today, industrial research is an important industry in its own right. It is a $7 billion-per-year business, accounting for 1.6% of our gross national product, and one in

which more than five hundred thousand people are employed. In fact, many research organizations would meet most of the definitions that are used to describe big business. There are today quite a few research organizations with more than three thousand employees conducting an annual business in excess of $50 million.

Since the modern industrial research organization is a relatively new phenomenon, it will be helpful to outline what such an organization is like. Briefly, my own company is not typical of all research organizations because there are still many ways in which research is organized, but it is representative of a form of organization that has been adopted by a number of companies and describing it will give some idea of what is involved as we try to carry out industrial research under today's competitive conditions.

Esso Research and Engineering Company is a member of what is known as the Jersey family. It is a separate company and was originally organized in 1919 to provide affiliates of the Standard Oil Company (New Jersey) with the technology that they needed to carry out their operations effectively. While there have been a number of corporate changes in our thirty-nine years of existence and while we have grown extensively since we were formed, the same basic idea has persisted and we continue to operate as a separate organization within the Jersey family. We receive no income from our parent; all of our income is the result of contracts negotiated with our affiliates for research in specified fields or the result of direct requests from affiliates for particular research and engineering data. Our operating programs, therefore, must recognize our anticipated revenue from long-range contracts and from what might be termed "spot business." This is particularly important in our case, since our basic contracts are of the net-expense type with stipulated maximum charges. This means that, according to the contracts, we cannot make money but we can operate at a loss. Since we have no reserves to cover losses, budgeting in this regard is very important

to us. Our expenditures exceed $50-million annually, and any major discrepancy between expense and revenue would be disastrous.

Organizationally, we are like most other businesses. We have an executive group consisting of our Board of Directors, an Executive Committee, and functional officers and their staffs. This group supervises and coordinates the operation of 18 divisions, 11 of which are engaged in carrying out research and engineering and 7 of which are service or supporting groups, including a Treasurer's Division and a Comptroller's Division. Each division has its own budget and is responsible for operating in accordance with it. Also, through a series of separate contracts, we purchase research results from other Jersey affiliates who operate research organizations under these contracts. Finally, we are the major repository of Jersey's patent assets in the refining and marketing fields and carry out a fairly large operation in the licensing of rights under these patents and in negotiating for rights under the patents of others.

I have included all of this to provide background for understanding that the development of a research budget under today's conditions is not just a question of how much money should be spent for research, but represents the development of an important working tool for research management. Our long-range budgets outline our program and cash requirements for both operating and capital expenditures. Our short-range or annual budgets represent an expression of research policy. In day-to-day operation, our budgets are important instruments of management control and, in retrospect, are a measure of our financial performance.

Under these conditions, the development of a research budget is carried out quite differently than many suppose. Ideally, it might be thought that the best way to arrive at a research budget would be to list ideas for research projects, select those ideas which appear to be good ones, come to some decision on the cost of the effort to be expended on each project, and, finally, add all these costs up to obtain the research budget.

Actually, the procedure is quite different although, when we get all through, the final results are not far different from meeting the requirements of the ideal situation. Our approach has to be different because a research organization is an extremely complex machine and takes far longer to build than most other corporate assets such as a new factory. In most companies, today's research organization is the result of plans developed and carried out in previous years. Today's plans do not determine today's research organization, but, rather, the organization of tomorrow.

Because of these considerations, the starting point in our budget procedures is the long-range budget. In our case, this represents our planning for the next ten years. The preparation of the long-range budget, as can be imagined, is a rather complicated affair that requires, on our part, careful consideration of our affiliates' long-range objectives in terms of (*1*) growth along established lines, (*2*) plans for diversification, (*3*) anticipated activity on the part of competitors, (*4*) long-range cash availability, and (*5*) the skill and attitude of operating organizations with regard to the use of technology. This can only be done on the basis of an intimate knowledge of the Jersey organization's current operations and plans for the future, and requires the closest type of cooperation between the research management and the management of the various affiliated companies.

On the basis of these analyses, it is, then, possible to designate areas in which research support is required and to evaluate the extent of this support over the years, area by area. These projections for individual areas are then brought together to arrive at our long-range forecasts in terms of total effort.

At this point it should be emphasized that a long-range budget is not a project budget in which we outline year by year exactly what we shall be doing. It is, instead, a forecast of the over-all magnitude of effort that will be required and of the general type of work that will have to be done. It is readily seen that, in the development of a long-range research budget, it is necessary

to assume that there will be more than enough good ideas for particular projects in each area and that a possible lack of suitable projects will represent no problem. Based on our past experience and that of others, this assumption is not particularly risky, since a competent research organization can always develop many more good projects than it has the resources to pursue.

In forecasts such as these, a high degree of accuracy is difficult to achieve. However, for the purposes of these budgets, extreme accuracy is not essential, since the forecasts can be reexamined and improved year by year. Furthermore, such budgets represent forecasts of probable needs or situations that will have to be met, and do not represent actual commitments beyond some 15–20% of the difference between current operation and the ten-year goal.

These long-range forecasts then represent our budget master plan. They outline both capital expenditures and operating expenditures year by year. They also outline our organizational requirements and serve as the basis for the development of year-by-year plans for organizational changes and training programs. From these plans we can then develop our cash-flow position year by year in terms of expenditures, income, and financing requirements. We can also develop plans for the employment and training of the scientists and engineers that will be needed to meet these plans; this is an important part of our budget procedure.

Once our master plan has been developed, the next problem is the preparation of the annual budget. In preparing the annual budget it is unrealistic to assume that we must head for our long-range projections along a straight line. In achieving the objectives of the long-range projections we need to retain enough flexibility in the preparation of the annual budgets to consider current as well as future problems of our own and of the operating affiliates.

One of the major considerations in developing the annual budget is to avoid action that will disrupt the possibility of achieving long-range objectives in a reasonably orderly fashion. For example, a forecast of long-range requirements in terms of labo-

ratories and offices will often prevent the construction of these facilities on sites which are too small and can not be expanded to meet long-range needs. We had some experience along these lines in the days when we did not use long-range projections, and I am sure that there is nothing so aggravating as the sudden realization that a building has been erected in the wrong place. However, when such problems are recognized, it is quite possible to achieve long-range solutions which may represent some temporary inconvenience but will, in time, provide the facilities needed for efficient operation in the future.

Another consideration applicable to operating budgets is that it is very undesirable to have wide swings in hiring programs. Not only are these swings extremely disruptive from the standpoint of morale but, in addition, they make development of a stable and effective organization difficult. Recognition of long-range needs can also go a long way toward preventing shifts in internal organization and in work assignments that must be retraced in the future to the detriment of all concerned. This is particularly true when an organization is tempted to discontinue long-range research in order to solve pressing current problems, since the long-range work is not readily picked up again; then, at some time in the future, the organization may find itself without the building blocks needed to support its then current programs.

Long-range or basic research is usually the most difficult part of a research program for the research organization to justify to others. It is also the most important factor in determining the future strength of the organization and must be vigorously protected at an adequate level. This level in our case amounts to some twenty per cent of total research and development technical personnel.

Generally speaking, a research organization has the least flexibility in terms of the over-all effort it can generate, since this is limited by physical facilities and trained people in the organization. This is the concern of the long-range budget and is a major reason for its importance. Somewhat more flexibility

exists with regard to the internal distribution of effort among areas of research, since it is usually possible to secure some interchange of facilities and people. The primary purpose of the annual budget is to outline the distribution of effort felt desirable for the coming year.

Maximum flexibility exists with respect to individual projects, since these can, within limits, be initiated, expanded, or stopped during the course of the year. Greatest operating efficiency is achieved when this is done to meet new requirements and is provided for in the annual budget by defining budget expenditures in terms of programs rather than projects.

With these considerations in mind, our approach to our annual budget is to start with the long-range budget or master plan. We then establish reasonable limits within which the annual budget can be varied without disruptive effect on the master plan. Then, on the basis of reviews of the needs of our affiliates, we determine what seem to be desirable changes in the emphases between areas of work within the over-all budget. The next step is to describe and rate in importance individual programs in each area. Following this, we try to fit into the tentatively established limits the programs on which work seems desirable. This inevitably requires that work on some programs be reduced. The next step is to make careful comparisons of the relative importance of programs reduced in each area and then try to readjust the areas so that they are in reasonable balance with each other.

At this point, the budget process returns to the over-all budget: we must now evaluate the desirability of exceeding or reducing the amount of effort indicated by the long-range bench marks. This evaluation is made primarily on the basis of pressures to do work on problems that cannot be accommodated in the annual budget as so far developed and the desire or ability of the affiliated companies to support programs of various magnitudes. These considerations can result in either a reduction or an expansion of the budgeted annual effort. The adjustment of the budget to the com-

pany's current needs is the result of a whole series of balances up and down the line. This is probably the weakest point in our budget procedure, because the answers depend almost entirely on judgment and are very difficult to determine quantitatively.

In our company, the budget procedure I have just described requires some eight months. It begins in April when our Executive Committee, after reviews of our technical problems and discussions with the affiliated and parent companies, sets up a tentative over-all budget figure and tentative figures for the distribution of the effort between areas. During May and June the individual divisions and the officers of the company having functional responsibilities work up individual programs to meet the general objectives. By September this is all brought together by discussions among the functional officers. The formal budget is then prepared by the Comptroller's Division for presentation to the Esso Research and Engineering Board for review in October. Finally, the budget adopted by the Esso Research and Engineering Board is presented to the parent company and to affiliates with whom we have contracts for further review and discussion. These discussions determine whether or not the Esso Research and Engineering Board wishes to reconsider its October budget action. Whatever action is required is taken during December and the budget for the following calendar year has then been completed.

I would now like to discuss the budget as an instrument of management control, which is another important use of the budget. Budgets are serious business with us; we expect our division heads to stay within their budgets, and we expect our officers having functional responsibilities to see that the distribution of effort in various areas follows budget planning. This problem of staying within the budget in a research organization is further complicated by the fact that salaries, wages, and related charges constitute our major items of expense. From a practical standpoint, dollar control can only be achieved by setting up a parallel budget for technical and nontechnical manpower and insisting on as careful adherence to this budget as to the dollar budget.

We have been quite successful along these lines but are faced, at this point, with a major problem that applies particularly to research organizations. This problem arises from the fact that it is extremely difficult to measure research productivity. It is one thing to control expenditures according to a budgeted plan, but it is quite another thing to know whether or not you are making or losing money by following the plan. That is, it is entirely possible for a division manager to stay within his budget by economizing in areas that destroy his technical output, but it is hard for the division manager or anyone else to know that this has actually happened.

We are trying to approach this problem from two sides. First of all, we are trying to develop methods for measuring technical productivity. This may sound like an almost impossible task, but we think it can be done, at least within much narrower limits than are now possible. We have a task force assigned to this problem and will continue to work until we arrive at a more satisfactory solution. Our second point of attack is in connection with services provided the technical people to make their work more effective. These include such items as laboratory analyses, engine test work, pilot-plant operations, laboratory assistance, and library, clerical, and stenographic help. Here we can do quite a bit in developing techniques for performing each service at the lowest possible cost and by setting up a reasonable price list for these services. If this price list is accompanied by individual budgets for the research teams, competition is established between the services available, and the technical groups are encouraged to select those services which represent the most economical support for their purposes. This is an area where sound budgeting and sound accounting practices are all-important, and we are convinced that management control of this nature can only follow from the establishment of realistic budgets.

As a final point, I should like to point out that our budget is an effective measure of the financial responsibility of research management. Such a budget not only indicates the ability of

the management to plan an adequate research strategy for the company but also, by comparisons between budgets and actual performance, it acts as a good index to the soundness of the plans themselves and of the extent of management control over its operations.

The following three principles I believe important in connection with research budgets.

Industrial research today is not a staff activity but is an operation that is engaged in creating a product. This product is technology, which is a tool to be purchased and used in other areas of the company's operations.

Second, successful management of industrial research requires careful analysis of the need or market for its products. It requires careful planning to meet these needs and careful control over costs if its products are to be created at a price that will make them useful.

Finally, the quantitative expression of all of these objectives is represented by the development of the research budget. Budgets of modern research organizations do not merely represent allowances for research expenditures, but express the long-range plans of a company to provide itself with effective technology over the years and to provide itself with research programs that will achieve these objectives.

ORGANIZING FOR PROFIT WITH NEW PRODUCTS. CARBOPOL POLYMERS: A CASE HISTORY*

FRANK K. SCHOENFELD
Vice President, Research, The B. F. Goodrich Company
Brecksville, Ohio

If a product can no longer be improved, its profit potential degenerates rapidly. Product technical maturity intensifies competition and forces the manufacturer to bolster his profits by imaginative merchandising and careful control. Since these are difficult business roads to travel, progressive industrial management looks to new products for rapid and profitable growth.

In the past decade, competition in many industries has become primarily a technical battle. The rapid rise in industrial research and development expenditures is the obvious result of this shifting of competition to the laboratories. Collaterally, increased technical competition has been an important factor in increasing the costs of developing new products and in decreasing the time period during which satisfactory profits can be obtained from a new or improved product. By necessity, American industry has been forced to become more sophisticated in organizing for creative obsolescence.

In this area, management does only half of the job when it determines the technical budgets and decides what expansion

* Paper presented at the Annual Spring Meeting of the Industrial Research Institute, Colorado Springs, Colorado, May 18–21, 1958.

capital can be made available. In order to clarify the total product strategy of an organization, management also has the difficult job of defining the scope of technical interests and integrating the included areas of scientific interest into the total needs of the business. More simply, new and improved products are a top management responsibility.

At the B. F. Goodrich Company, a policy of encouragement and control of product development has evolved over the years and is still evolving. Within corporate policy, the responsibility and authority for operations is vested in the divisional presidents and general managers. Staff officers and executives consult, advise, co-ordinate, and offer specialized services. Central Research is a corporate activity, whereas development is assigned to the operating divisions. In a real sense, the operating divisions are customers of Central Research. The research staff endeavors to have the operating divisions understand, appreciate, and use the available research facilities and skills. In addition to doing what might be termed contract research for the operating divisions, the Central Research group has "freedom-of-action" funds for studies and developments in the corporate interest.

The corporate officers and any divisional president involved form a Products Development Policy Committee to screen product ideas and follow on a regular schedule their progress through development and commercialization. This committee has a full-time secretary with a legal and technical education and with experience in patent law and research supervision as well as training in business administration. The various operating divisions have development, merchandising, and market-survey groups in accordance with their needs. There is also a corporate Business Research Dapartment.

No single product development could illustrate and clarify all the techniques that have been used in applying the corporate assets to upgrade profit potentials through product development. The development of a class of new materials, all of which are identified by the trade-mark Carbopol, has been chosen for our

case-study because it illustrates most of the techniques we use. Also, the Carbopols are a successful development. A development that failed might be even more educational, but certainly not as pleasant to discuss.

The Carbopols are synthetic hydrophilic colloids designed to meet the needs of the cosmetic, pharmaceutical, paint, and related industries for water-soluble resins that can reproducibly thicken, suspend, disperse, and emulsify.

A research project, started in the B. F. Goodrich laboratories before World War II, bore fruit in the middle 40's in the form of the versatile chemical, beta propiolactone. From this new intermediate sprang an impressive line of derivatives, among which was the well-known but little-used acrylic acid. Further, the process produced this monomer in a state of high chemical purity that was unknown to laboratory processes of the time. Accordingly, Research Administration had the economics of a commercial plant defined and, since this appeared favorable, initiated a polymer research program in an effort to capitalize on our favorable patent position.

Our company has been engaged in the field of synthetic high polymers for a number of years, and its products find uses in many varied fields. However, the polymers are useful in all these fields because of their solid-state properties. They are all structural materials. The initial studies with the new monomer were discouraging. In its new, highly purified state, it could be polymerized either by itself or with other monomers. However, the polymers thus obtained did not lend themselves to any of the structural end-uses with which we were familiar. They were quite water-sensitive and, depending on their exact method of preparation, were either water-soluble or water-swellable.

It soon became apparent to our research personnel that these polymers comprised a new class of synthetic hydrophilic colloids resembling most closely certain of the natural gums, with the difference that they were much more effective in the thickening of aqueous systems. As the polymer work progressed, several

monomers in addition to acrylic acid were introduced into our program. Certain of the polymer products, when properly treated, were from 15 to 20 times more effective than the natural materials, i.e., one gram of the synthetic polymer would thicken water to a degree that required 15 to 20 grams of a natural gum. Other modifications of the polymer, on the other hand, were not nearly so effective as thickening agents. These formed aqueous solutions of intermediate viscosity that exhibited the property of suspending solid particles or emulsifying immiscible oils in water. Still other modifications served to bond inert materials, such as sand, into coherent masses.

The varied characteristics of these compounds, together with the lack of sufficient background within the company in these fields, convinced Research Administration that to find suitable applications for these products it would be necessary to work closely with Sales. Direct customer contacts would be needed for the purpose of arriving at appropriate evaluations of even these early developmental products. Consequently, the research staff made a presentation to the corporate officers, using statistical information from the corporate business-research group to define roughly the magnitude of the product field within which we were working. The legal department's evaluation of our patent position also was detailed.

Because of the unique properties of these materials, the possible good market, and the enthusiasm of research personnel, the corporate officers agreed to a continued study. Since the research group was most familiar with the situation, it was agreed that further evaluation would be centered in that group for the time being but, at the same time, it was agreed that other divisions within the company would be called on to contribute whatever help was needed to aid in the evaluation.

The early product development was, therefore, centered in Research, which took the lead in both product preparation and evaluation. The established sales-service and product-application laboratories in the company were completely without ex-

perience in the fields opened by these new polymers, and these groups were therefore not called on at this time. The B. F. Goodrich Chemical Company, however, because of its customer contacts and its ability to conduct market surveys in fields of this type, was called on, and there developed on this product the close co-operative effort that is normal between this division and Research.

In a number of instances, the only meaningful method of evaluation was by means of trial in a product. For example, the only method existing for the evaluation of a hydrophilic colloid for use in a toothpaste formulation was to make and to evaluate the toothpaste. The same could be said for an automobile polish, a hand cream, a printing ink, or a drilling mud. The product-development philosophy was based, at this time, on the recognized similarity of the product to the natural gums, and research effort was directed toward structural modifications of the polymers to duplicate the properties of these gums. It was soon recognized, however, that these new synthetic hydrophilics possessed unique colloidal characteristics of their own and probably would find special applications which no other material, either natural or synthetic, could fill.

By this time the Chemical Company had completed its first market survey which indicated that it would be difficult to compete with the cheaper natural gums, such as guar gum and karaya, but that we could compete, on an economic basis, with the more expensive natural products such as gum tragacanth. Further, some obvious advantages existed over the natural gum tragacanth such as superior stability toward hydrolysis, and resistance to attack by bacteria and molds. Also, there was an economic advantage in that the best grade of gum tragacanth was then selling for $4 per pound. The survey revealed that some 1.5 million pounds of tragacanth were imported annually from the Middle East where it is gathered as an exudate from a shrub indigenous to that area. The market survey defined the applications of gum tragacanth so that contacts could be made with some of the

principal consumers. At this point, the sales and research groups operated as a team, making calls on tragacanth users.

As it turned out, the fundamental soundness of this approach was confirmed. Few, if any, of the measurable physical properties of gum tragacanth are considered with respect to application. Specifications are vague, and the gum is sold according to grade, which is an arbitrary standard depending more upon color and cleanliness than upon aqueous mucilage characteristics. Generally, the customer was sufficiently impressed by the appearance and the much enhanced mucilage characteristics of the synthetic gum to request a sample for formulation trials. The customer usually gave us his typical test recipes from which formulations could be made for our studies. In most instances, this procedure was very unscientific and hardly appropriate for research personnel, but at this stage it was the only course available.

With the development of specific polymer compositions capable of entering successful end-use product formulations, research was faced with the problem of process development. This problem arose, not only because of the need for more accurate engineering studies and cost analyses but also because of the need for larger samples for more complete product evaluation.

It was at this point in the development that the whole product strategy was critically reviewed. Management's Product Policy Committee asked itself the question: "Do our research, development, engineering, construction, and production divisions have the necessary experience and the necessary ability to handle this new product?" In other words, could we make this product efficiently? It was decided that our Chemical Company did have the abilities to produce this product. Further, while the initial phases of process development had been done by research personnel, the similarity of the polymerization process to others used by the Chemical Company made it possible to save time and money through omitting research pilot-plant operations. The process, therefore, went directly to the manufacturing process-development

group at the Chemical Company's Development Center at Avon Lake, Ohio.

Secondly, our sales competence was reviewed. Did our advertising department know how to handle the promotion of this particular type of new product? Did we know the trade practices in this market? Did our normal selling techniques fit in with the established practices in the market where this new product must compete? We were asking ourselves if we could interest others permanently in what we planned to do.

It was decided that no operating division other than the Chemical Company would have an interest in this material. This simplified co-ordination of the project since only the research staff and the Chemical Company would be involved. The Chemical Company was confident they had the sales competence to do the job, but did wish to use the services of the corporate staff's advertising and merchandising experts for advice and consultation. Further, the Chemical Company would underwrite the future research costs of hydrophilic-colloid projects which previously had been supported by research "freedom-of-action" funds.

The economic soundness of the project could be only partially evaluated. Raw materials were readily available. The costs of producing and selling could only be approximated but appeared to be compatible with obtainable market prices. The market situation seemed satisfactory. While this was a new product, it did not require the development of a new market, for it replaced existing natural gums that enjoyed an attractive dollar sales volume. The users of natural gums were unhappy with the variability and the uncleanliness of the natural products. In addition, the properties of our synthetics were so superior that there was a good chance that the markets could be expanded.

The introduction of the Chemical Company's process development group to the project closed a cycle of development, because this same group had by now also become firmly involved in developing the chemical process for the new monomer that had opened the field of hydrophilic polymers to the company. Members of

the process development group had already carried along the development of the monomer process to a point where commercialization was feasible. It was now not only possible to prepare the monomer in a satisfactory manner, but other groups in research and chemical development had enlarged the scope of the chemical reactions involved to the point where other chemical products and intermediates could be made. The hydrophilic polymer program thus fitted into the over-all chemical picture that the B. F. Goodrich Company was developing from its original research discovery.

With the Chemical Company taking over responsibility, the market-development work of its sales department was speeded up. Contacts were established with the major users of hydrophilic materials of all types, and those using gum tragacanth were especially surveyed because it was felt that this represented the quickest route to success. Contracts were made with an outside consulting laboratory for product studies and market appraisals, and with a university laboratory for a specific end-use study. At the same time, because it appeared that pharmaceuticals and cosmetics represented potentially large fields of use, toxicity studies of the polymers were initiated. The successful completion of these tests opened the way for further contacts in these fields. At this date, while acceptance has been gained for ingestion in quantities sufficient for use in pharmaceutical preparations, sufficient studies to permit use in foods still have to be completed.

As the sales and development programs emerged from their infancy, groups were organized at the B. F. Goodrich Chemical Company's Development Center to take over product evaluation and application studies. These groups are appropriately known as New Products Evaluation and Products Application. Actually, these organizations were already in existence and functioning on other chemical and polymer problems so their functions were merely expanded in order to embrace hydrophilic polymers. This immediately took a burden from Research and allowed it to resume its normal function. Even then, however, the novel

character of the product with respect to B. F. Goodrich experience required the maintenance of more intimate contact between Research and Sales than would exist normally. This was required more for the edification of Research than for assistance to Sales.

At this stage in the development, an unanticipated difficulty developed. While many of the potential customers were willing and, in some cases, eager to test our materials as possible replacements for natural gums in their formulations, they were equally reluctant, at the point of sale, to change an established recipe for a price advantage or for the assurance of improved uniformity of material. It was therefore necessary to demonstrate that our materials would add to the customers' products advantages such as ease of application, stability to aging, and freedom from odor and bacterial attack. This was accomplished by extensive formulation work and complete rheological studies of our polymers. This work was done primarily by the New Products Evaluation and Products Application groups, and, in certain instances, by the laboratories of our customers.

These studies served, in most cases, to overcome the reluctance of the customer to use our materials. More importantly, they served to change our position from one of viewing the materials as substitutes for natural products to one of viewing them as a completely new class of materials capable of entering new fields of use where natural products had not previously been used. Our hydrophilic polymers were taking their place in the field and were becoming capable of standing alone.

Sales were beginning to approach the capacity of the semiworks plant that had been constructed by the Process Development group. Consequently, at the periodic review, management authorized enlargement of the semiworks facility and engineering of a production plant was started.

Our program on hydrophilic polymers reached maturity about two years ago. The trade-mark, Carbopol, was adopted for the family of commercial materials, and a fully developed sales program was organized. Since that time, the research group has

resumed a more or less normal role designed to expand the base of our hydrophilic polymer business and to search for product and process improvements. Inasmuch as the business is still in an expanding state, the research budget is larger than the immediate sales volume would normally justify.

The New Products Evaluation group now is charged with the responsibility of expanding the usefulness of our existing line of Carbopol products. This is done by exploring new uses based on the intrinsic properties of individual polymers. Also evaluated in this respect are new materials which come from Research. For example, each polymer is examined critically as a thickener, dispersing agent, emulsifying agent, flocculant, etc., under a variety of different conditions and in a number of different environments. It is from these studies that data are compiled for sales bulletins and brochures.

The Products Application group is primarily concerned with sales service. These people study customer requirements and adapt our products to their needs. This is primarily formulation work, and it may be based on formulary recipes or recipes suggested by the customer. The usual field contacts are made with sales and the handling of customer problems is normal for this type of function.

The Process Development group performs its normal operation. A direct contact with the research laboratory is still maintained. When a new polymer is passed from Process Development to Production, the development engineers move over to the production plant and stay with the process until both process and product specifications are attained. This is more or less normal with us for this type of operation. New product preparation for customer sampling is one of the responsibilities of this group.

Rather more than the usual amount of engineering skill is required of this group of engineers. Hydrophilic polymers find their applications through their role as colloids. Hence, molecular structure and the routine reproduction of this structure in the process is of basic importance. Also, in some of our Carbopol

polymers, particle size is fundamental with respect to colloidal properties. It requires the highest degree of engineering skill to maintain reaction conditions which will give reproducibility from batch to batch. The problem is particularly complex in the expansion of reactor size.

The function of the sales group in our present status is nearly that of any other well-organized sales organization. The rather great diversity of applications of hydrophilic polymers demands a high degree of versatility on the part of our salesmen, who, incidentally, all possess technical degrees. The salesman must be alert to customer needs and must be better informed than most with respect to experimental materials which we have in the background. Also, it is important that a good line of communication should run from the customer all the way back to Research, for we are still working in a highly undeveloped field, technically. Naturally, the salesman is the anchor man in this chain of communication.

The empirical character of the applications of hydrophilic polymers makes necessary a somewhat different relationship between Sales and the customer. The product is sold more on its performance in a given recipe than on the basis of a set of well-defined physical or chemical properties. For this reason, customer relations must be maintained on a more intimate basis than is required for chemicals or structural polymers for which definite and meaningful data can be published. Advertising and sales literature must be more graphic and more illustrative than is normally found.

To expedite communications within the company there has been organized a Steering Committee on Hydrophilic Polymers. The Sales Department was the motivating force in the organization of this committee as it felt most acutely the need for speedy and accurate communications. This committee consists of representatives of the previously described groups associated with the Hydrophilic Polymer program. These are: Sales, Products Application, New Products Evaluation, Process Development, Production, and Research. The committee meets monthly and reviews the

status of the various individual programs within the group. Even the research program is reviewed, although at this stage it seldom is pertinent to the immediate problems of the other groups. For the most part, the research member serves in a consulting capacity. The presence of a production member on the Committee is essential because of the relatively delicate reaction control required to maintain customer product specifications. Also, customer complaints of most types impinge on production in one way or another.

The Steering Committee actually has no power to direct, even with respect to its own members. However, its recommendations are given most serious consideration by the departments represented and management in general. Typical items for Committee discussion are new sales fields to be considered, possible new products, testing procedures, specifications, and customer complaints. While this committee was created because of mutual inexperience in a new field of endeavor, it appears that, from the standpoint of communications alone, its existence is more than justified. Such advisory groups have become standard in our corporate product development technique and are beginning to assume responsibilities for following established product lines.

The development of a line of hydrophilic polymers has been scientifically interesting and also profitable to our company. The profitable aspects have by no means been limited to return on investment. The lessons learned and the techniques acquired have improved our abilities to organize for profit with new products.

HISTORY OF THE DEVELOPMENT OF PYROCERAM*

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Pyroceram is Corning's trade-mark for products made from a new family of materials called glass-ceramics. Glass-ceramics are made by first melting and forming special glasses containing nucleating agents and then causing controlled crystallization of the glass particles. The resulting products are hard, strong, crystalline materials whose properties are functions of the crystal composition and the process. This paper describes the general philosophy that governed the research leading to the development of Pyroceram. The history of the development is outlined, and the outstanding characteristics of one of this new family of materials is summarized. In conclusion, those factors in the research environment which are believed to be necessary for successfully carrying out a long-range research program such as that which led to Pyroceram, are listed.

The Basic Scientific Principles

The scientific principles on which glass-ceramics are based are quite simple. These principles are set forth on the next page:

* Paper presented at the Annual Spring Meeting of the Industrial Research Institute Colorado Springs, Colorado, May 18–21, 1958.

1. Glass at room temperature is an undercooled liquid. It has not reached the stable crystalline state.

2. Glass can be made to crystallize in a temperature range below the equilibrium melting point if crystal nuclei are present.

3. Controlled crystallization of glass to a useful, uniform, fine-grained crystalline material can be accomplished by the use of heterogeneous nucleation. This is done most effectively by introduction of nucleating agents that precipitate as a highly dispersed phase as the glass is cooled or reheated.

The third principle was not developed, as it might have been, by a simple process of logical deduction from the first two. Instead, it was recognized only after a long and involved series of experiments (started, incidentally, with no idea of producing anything like Pyroceram). Luck played an important part in controlling the direction of the work.

The main stream of this research program has been exploratory or pioneering basic research, although it has had feeder and outlet streams of pure fundamental research and development work, of which the latter, as might be expected, has involved much more expense and personnel than the exploratory research. For much of the time, the direction and execution of this exploratory research has been a one-man effort, although this must be qualified by emphasizing that the results have depended on free access to the advice and past experience of all of the research people in the Corning Laboratory, and all of the tools, resources, and service groups of the laboratory.

Recently, the basic research group employed in this study has been expanded; however, the "one-man" idea is being maintained, in that each of these men is working independently of the others in a research area offering sufficient scope for creative achievements.

Photosensitive Colored and Opal Glasses

The background of the development of Pyroceram can be traced back at least a hundred years to the discoveries of gold and

copper ruby glasses and of opal glasses. Copper ruby glass contains colloidal copper, precipitated from homogeneous glass by cooling and reheating. Some opal glasses (white, opaque glasses) owe their opacity to crystallites of fluorides or phosphates precipitated similarly from a homogeneous melt by cooling, or by cooling and reheating the glass.

In 1937 at Corning, Dr. Robert H. Dalton was investigating methods of producing controlled color patterns in glass. Knowing that exposure to sunlight changes the color of some glasses, he tried exposing a copper-containing glass to ultraviolet light and found that subsequent heat treatment produced a more intense red color in exposed glass than in unexposed glass.

Three years later, when I came to Corning, my research supervisor, Mr. H. P. Hood, first assigned to me the problem of exploring a certain three-component composition field of fluoride opal glasses. From that point on, he allowed me complete freedom in selecting and carrying out my own research program, while giving me any advice and help that I wanted. I had become intrigued by Dr. Dalton's observation of photosensitivity, and chose to attempt to control the precipitation of crystals in a sodium fluoride opal glass by irradiation and heat treatment.

The first attempts to produce a photosensitive opal glass were completely unsuccessful. It was therefore considered advisable to learn more about the reasons for the behavior of the copper ruby glass. Investigation proved that the reaction was very sensitive to the state of oxidation of the copper and actually was a photochemical and thermal reduction of cuprous ions to colloidal copper and, furthermore, that the effective ultraviolet radiation was absorbed by the dissolved copper ions. We were able to improve the composition sufficiently so that good photoimages could be produced in color, leaving the unexposed glass colorless.

This study explained our failure to control the precipitation of sodium fluoride crystals by radiation, since this case involves no oxidation-reduction reaction.

The next step was to investigate the gold ruby glasses. The kinetics of the precipitation were found to be more complex than had been previously recognized. Rather than a simple temperature-sensitive solubility curve for metallic gold, it was found that the gold dissolved as ions, and that other multivalent ions, such as those of tin, antimony, or selenium, caused reduction of the gold ions to metallic gold at low temperatures.

We then learned how to make a photosensitive gold glass. This necessitated removing the multivalent ions mentioned above, and substituting cerium oxide as an optical sensitizer. The cerium was found necessary since gold ions evidently did not absorb the near ultraviolet radiation as did copper ions.

When it had been shown that colored photosensitive glasses could be produced in production-scale melting units, management was faced with a decision as to whether and how these glasses might be marketed. Since all of the applications appeared to be decorative rather than functional, it was decided to license processors, selling them the unprocessed polished plate glass.

Remembering our first goal—that is, to produce a photosensitive opal glass—it occurred to us that the colloidal metal crystals, if they were precipitated in appropriate glasses, might nucleate crystallization of other constituents of the glass. This proved successful in some special glasses.

The first photosensitive opals were lithium silicates. However, these were not considered practical because of their cost, and an inexpensive sodium-fluoride opal glass that was sensitized by gold was developed. This glass, which required a double heat treatment to produce crystallization, was discovered partly by a chance observation.

Here again, management was faced with a decision regarding market possibilities. It was decided that a potential market in flat glass—lighting ware, appliances, architectural applications—might be developed. It was further decided to risk a pilot run in a full-scale continuous tank. At this point, I was confronted with a difficult problem. Only a few weeks before the melt was scheduled

to start, I discovered that silver could be used instead of gold to nucleate the sodium-fluoride opal, and that this was not only a less expensive glass but also one that could be processed at a lower temperature and with much less deformation of the glass. Should we take a chance with the new glass, or play it safe with the glass that had been more thoroughly tested? The new glass was used, and fortunately it was successful.

Fotoform Glass

Up to this point, the research had all been of the "permissive" type. Here, for the first time, an element of directional control entered the picture. The Director of Research, in a general staff meeting, pointed out the potential value of being able to make economically a great many holes through glass, and asked the staff to try to develop new methods for doing this.

By this time we knew that the physical properties of some of the opals were different from those of the base glass, so we experimented to determine whether the chemical solubilities were different as well. We were happily surprised to find that the crystallized image in photosensitive lithium silicate opal, a glass which had been set aside as impractical, was many times more rapidly dissolved in hydrofluoric acid than the adjacent clear glass. This led to the development of Fotoform glass and chemical machining.

Here again we had a new basic material calling for management decisions as to applications and procedures. A product-development group was set up in the laboratory. Most of the potential applications appeared to be in the television and electronics industries, although other types of applications, including manufacture of printing plates, were investigated. Recently, Fotoform has graduated from the laboratory into a manufacturing division, the Electrical Products Division, and a new manufacturing plant is being built for it and other electrical components.

Most of the applications of Fotoform stem from its ability to be chemically machined into extremely complex and precise

shapes at relatively low cost. One market, for example, is in fine-mesh screens, some having as many as a million holes per square inch, for electronic devices.

Fotoform to Pyroceram

With Fotoform glass we first began to realize what profound changes could be brought about by a trace quantity of an effective nucleating agent. This realization was brought into sharp focus by an accident, which turned out to be one of the most important factors in the development of Pyroceram. This accident was the fault of a furnace temperature controller, which allowed the furnace to heat a plate of exposed Fotoform glass 300 °C. hotter than the control setting. Expecting to find a pool of melted glass when we opened the furnace, we found instead a plate of crystalline material that was much stronger and harder than the original glass. This material was called Fotoceram; it is a valuable material in itself. For example, its properties make it an excellent material for high-quality electrical circuit boards.

By this time it had become obvious that controlled crystallization in glass was both feasible and useful. The next stages in the development of Pyroceram involved the expansion of the knowledge gained in the particular experiments described above to a general theory including the three basic principles with which this paper began, the search for other nucleating agents, and their use in a wide range of compositions.

Since photosensitivity is not a necessary part of the nucleation process in many cases, the name Pyroceram was coined for these materials. This word was later adopted as a trade-mark designating products made from these glass-ceramics.

As soon as we realized how broad the range of glass-ceramic compositions might be, the nature of the research changed to a team effort, directed toward two goals. One of these was to explore the important composition fields and obtain the necessary information for preparing patent applications. The other was to

select a few promising compositions and begin the development of processes and products.

It happened that, coincident with the first crucible melts of experimental glass-ceramics, it was discovered that Fotoceram had greater strength and remarkably high resistance to high-speed impact compared to glasses and most ceramic materials. This immediately focussed attention on radomes for high-speed missiles. Some of the earliest glass-ceramics were found to have still higher strength and hardness, better thermal shock resistance, and better dielectric properties at radar frequencies than the Fotoceram. Tests by defense agencies verified that these glass-ceramics had good properties for radomes; so, although production was still in the crucible stage, a development contract to determine the feasibility of radome production and to obtain radomes for evaluation by the defense agencies was undertaken and successfully carried out. This involved pilot-plant production of radomes.

From this point on, as other compositions having new and useful combinations of properties were developed, it became necessary to establish product-development groups. Other company divisions outside the Laboratory began to take part, and the rapidly mushrooming number of problems became complex for all levels of management.

A wise decision was made to expand the fundamental research group and not to yield to the temptation to concentrate entirely on product development for immediate marketing.

A very difficult management problem was to decide when and how to make public the new developments. It was known that an invention of such complexity and broad scope would probably require years to perfect completely, so that immediate public announcement in some respects would be premature. On the other hand, incomplete and distorted stories were beginning to circulate through grapevines. The announcement was made, as you may know, at the time of the dedication of the new Corning Laboratory, on May 23, 1957.

The response to this announcement has amazed everyone at

Corning. A deluge of about 10,000 requests for information has resulted. In order to cope with these successfully and to tie together the activities of the various divisions of the Company in this field, a Pyroceram coordinator was appointed by top management.

The latest episode in the story of Pyroceram has been a successful run of a general-purpose glass-ceramic in a specially-rebuilt full-scale production tank. This glass-ceramic has been used for many kinds of articles from ball-bearings to pipe, skillets, sheet glass, and large telescope mirror blanks. This production represented a million-dollar gamble by the Company, with a glass that had to be formed 100 °C. hotter than any production glass made previously in addition to posing many other new problems for production.

Properties of a General-Purpose Glass-Ceramic

A description of the general-purpose glass-ceramic just mentioned will illustrate some of the properties of glass-ceramics and indicate the reasons for our interest in these materials.

The raw materials are relatively low in cost—a few cents per pound of product. The glass is capable of being melted and formed by inexpensive and versatile glass manufacturing techniques, with only a heat treatment required in addition to convert the glass article to glass-ceramic. This means that excellent reproducibility and close control of properties are possible.

The flexural strength is several times higher than that of glass. For abraded rods, it ranges from 25,000 to 35,000 psi, depending on the heat treatment given. The softening or deformation temperature is over 1200 °C.

The thermal expansion coefficient of this particular glass-ceramic is extremely low, about $8 \times 10^{-7}/°C$. This is almost as low as that of fused silica, so that the material is virtually immune to breaking by thermal shock. Also, dimensional changes with temperature are very small. Dimensional changes during conversion from glass to glass-ceramic are under 1%, in contrast to

about 15% for conventionally made ceramics, which are made by first forming a pressed or slip-cast body and then drying and sintering it until the grains are bonded to one another. Also, glass-ceramics are nonporous and much finer grained than conventional ceramics. These things contribute to their superior strength. The hardness of this glass-ceramic is superior to that of glass and of most metals.

One of the most interesting facts concerning glass-ceramics is the broad range of compositions that can be employed. This means that we are only at the beginning of this research, and that we can hope for great advances in strength, hardness, temperature resistance, and other properties.

The Necessary Research Environment

I am convinced that this research program would have been possible only in an environment unusually favorable for long-range research. Speaking from the viewpoint of a research man, some of the important factors in such a favorable environment appear to me to be as follows:

First, every member of the management from the Research Supervisor through the Research Director to the Chairman of the Board of Directors must be genuinely convinced of the value of long-range research and ready to support it actively.

Second, the atmosphere throughout the laboratory should be such that free interchange of information is possible, the research man is confident of recognition of his work, and constructive application of the results is made.

Third, the company should be large enough to support long-range research and make use of its results; but not so large that the research man is too small a cog in the machine.

And finally, promising research men should be given fruitful fields of study with sufficient scope, and given sufficient freedom, time, and independent responsibility to encourage their best efforts.

HOW RESEARCH ORGANIZATION AND PROGRAMS HAVE BEEN DEVELOPED IN A LARGE CORPORATION*

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Twenty years ago Union Carbide and Carbon Corporation, as it was then named, consisted of a number of companies which were essentially autonomous in the conduct of their business affairs, including research. The four principal companies were the National Carbon Company, The Electro Metallurgical Company, The Linde Air Products Company, and the Carbide and Carbon Chemicals Company. The several businesses differed considerably, and their research programs seemed to have little in common. Not all of the research organizations were represented in the top management by a vice president, and many of the research programs were short-term in character and development-oriented. The still young and vigorously growing Carbide and Carbon Chemicals Company was an exception, and several of Carbide's leaders, including Mr. J. A. Rafferty and Dr. G. O. Curme, Jr., were greatly encouraged by the performance of the Chemicals Company. These men were strong and persistent

* Paper presented at the Annual Spring Meeting of the Industrial Research Institute, Colorado Springs, Colorado, May 18–21, 1958.

proponents of imaginative research as a means of fostering industrial growth, and they used the experience of the Chemicals Company to persuade others in the Corporation of the validity of their views. Partly as a consequence and partly as a means of avoiding persuasion in the most difficult places, the concept of corporation-supported research, in contrast with a division-supported research program, came into being. This was in the late 'thirties. The idea was something like this. Research, especially exploratory and basic research, could help in the growth of all of the divisions, just as it had helped the Chemicals Company. In order to get programs started and at the same time not charge the individual companies directly for the research, it was proposed that the charges be picked up by the Corporation. This scheme won favor, and, in 1939, a new research organization was formed and housed in a new laboratory at Tonawanda, New York, along with the research staff of the Linde Air Products Company.

At the outset, the magnitude of this new research effort was rather small—in comparison with present-day activity it was minute—but it must be remembered that in 1939 the total spent in industrial research laboratories in the entire country was only $200,000,000. Today such laboratories are spending at roughly 25 times that rate. Not only was the new venture small but it also had to face all of the practical problems usually associated with the organization of a new laboratory. In addition, for a time at least, the curiosity of other groups in the Corporation and the scrutiny of the executives who fostered the idea bore down pretty heavily on the new research group.

The main problems were the recruiting of personnel and the formulation of programs. Actually, the two problems were closely linked. Some of the most imaginative scientists in each of the company laboratories were offered the opportunity to work in the new organization and were asked to suggest ideas for research, preferably research in fields outside of the Corporation's then present business interests.

These key men recruited their supporting personnel, mostly

from universities, and went to work on a program more or less of their own choosing. The studies they engaged in were, in the main, quite basic. The behavior of materials at extremes of temperature; molecular structure of materials; physics of solids; mechanism of crystal growth; relationship of crystal structure to adsorption properties; mechanism of selective adsorption of gases by crystals; the physics of arcs; the interaction of metals and metal oxides; the behavior of materials at very high pressures; the separation of volatile inorganic compounds by distillation and diffusion were typical topics. Inasmuch as the studies were mainly concerned with inorganic substances, the laboratory became known in Union Carbide as the "Inorganic Laboratory" and for a while the accountants designated its budget as the "Inorganic Budget."

As the operation progressed a number of interesting things occurred. The Linde Laboratory was the "landlord" and supplied laboratory management services for the "Inorganic Laboratory." This inclusion of one laboratory within another occasioned surprisingly little difficulty. The novelty of the program and the freedom associated with it were attractive to new recruits from the universities. The Linde Laboratory was able to take advantage of this in its own recruiting program.

One surprising thing that occurred was the development of a kind of secretiveness. In retrospect, it is fairly obvious that this was actually diffidence about talking about work in its very early stages, especially work which was difficult to associate directly with clearly defined business objectives. As a few research successes piled up, however, the cloak-and-dagger atmosphere began to dissipate, only to recur as some of the results had important bearing on technology "classified" by the Atomic Energy Commission or the Defense Department. For several years, communications from the "Inorganic Laboratory" to other laboratories in the corporation were somewhat guarded. With the growth of the program, the growth of the Linde Laboratory, and the ending of the war, this difficulty was gradually resolved.

Meanwhile, some very useful things came out of this basic research. For example, the study of very-high-pressure techniques led to an invaluable knowledge of equipment design and to manipulating skills that, in turn, led to Carbide's leadership in the commercial production of polyethylene—first for radar and subsequently for civilian consumption. The study of the growth of crystals led to commercial synthesis of sapphires and other crystals, during the war for instrument bearings and optical shapes and subsequently for gems.

The laboratory was able to make important contributions to the Atomic Energy program as a result of its work on the chemistry of uranium and on the methods of separating volatile inorganic substances. Crystal-structure studies led ultimately to the synthesis of an important class of inorganic adsorbants called molecular sieves. The fundamental study of arcs provided basic information of tremendous importance to metal processing and fabrication and to high-temperature chemical processes.

As these and other things developed during the early '40s, more and more attention was paid to the program by the other laboratories in the Corporation. Since, by reason of its location, the "Inorganic Laboratory," tended to be identified with The Linde Air Products Company, the other divisions began to feel left out, and, as their programs expanded, they began to include some "blue sky" research. At first, some of the new topics which appeared in the budget proposals caused the eyebrows of some of the division presidents to lift and such questions as "Why are we doing this?" and "How will this ever fit into our business?" were raised. But a start was made, nevertheless, and where approvals were slow in forthcoming there was some occasional "bootlegging."

Then, in 1944, an event occurred which was to have a profound effect on the Corporation attitude toward basic research. This was the formation of the Union Carbide Research Committee, headed by a vice president of the Corporation and comprising the several vice presidents in charge of research for the principal divisions, including Carbide and Carbon Chemicals Company, the

Electro Metallurgical Company, The Linde Air Products Company, the National Carbon Company, and the Bakelite Company. This committee began to knit the several research programs together. It encouraged lateral communications and reviewed and approved programs and budgets. The Committee met at regular and frequent intervals, often in the laboratories. This manifestation of interest on the part of top management was quite encouraging to the scientists. At the same time, the committee members came to a better understanding of the objectives and administrative problems of their fellows. In due course, it was recognized that there was a certain similarity between the problems of the several laboratories and that each laboratory had a background of experience that might be shared profitably.

Accordingly, several subcommittees were set up with members from each of the companies. One subcommittee was charged with studying laboratory management practices with a view toward selecting the best and using them wherever applicable. Another was charged with broadening the scope of our scientific program and encouraging the production of new ideas—fostering creativity, if you will. It was difficult to find a mechanism for doing the latter, and several years went by before the subcommittee felt that it had made substantial progress.

One of the most effective things the Research Committee did was to get our scientists acquainted with each other. They started out by holding a series of meetings in the several laboratories. They invited three, four, or five scientists from each of the laboratories to convene and hear a review of the research program being conducted at the host laboratory, after which they would have a social get-together. The Committee was encouraged by the results, but not greatly so. They found that, while the participants were quite interested in what their counterparts in other laboratories were doing, the general reviews of the complete research programs were not sufficiently detailed to permit an exchange of views on specific problems. The most encouraging aspect was that a number of imaginative and highly

productive people got acquainted with each other. Subsequently, the Committee decided to hold similar symposia but to limit the subject matter to particular phases of research endeavor. This permitted full and complete discussion of the detailed scientific aspects of particular projects, which seemed to be exactly what was desired by the scientists. From these meetings there evolved a real acquaintanceship, and men with similar scientific interests, in different laboratories, kept in touch with each other about their respective progress. They also stimulated each other; this was provocative of new ideas.

The result was that a number of new projects were suggested, most of which were basic and exploratory in character. These suggestions were put forth in the form of proposals for research projects in ensuing years and, with the encouragement and approval of the Research Committee, they became part of what we now call our Corporation Research Program. The work is actually done in the division laboratories. The proposals come from the young scientists. They are screened by their own laboratory managements and submitted to the Research Committee for approval. The individual companies, or divisions as we now call them, are not charged directly for the financial support of these projects; a fact which removes one possible bar to their approval. Under this procedure, the Inorganic Laboratory at Linde which had pioneered in basic research in the corporation became simply that portion of the Linde divisional laboratory engaged in Corporation-supported research under the Corporation Research Program.

I believe it will be of interest in this connection to review the manner in which our research budgets come into being. Each September, the local management of each individual laboratory requests its scientists to propose research projects for the next year's budget. As you know, a large proportion of most research programs consists of topics which are essentially identical with those of the previous years' programs. There are always items which should be and are dropped, and these are replaced with new projects. In an expanding program there are additions, as well.

After a screening of the scientists' proposals by the local research management, a draft of the budget proposal is submitted to the vice president in charge of research for that division. He reviews it and then, as a member of the Research Committee, submits it to the full committee for a comprehensive review and critical discussion. The criticisms and suggestions offered by the committee are then taken as a basis for whatever revision of the budget proposal may be made when the division vice president reviews his budget with the president and other officers of his division. Subsequently, the several budgets are submitted by the presidents of the divisions to the Corporation Appropriations Committee for approval. At this stage, the basic and exploratory work which is to be supported by Corporation funds directly, rather than by division funds, is shown in a separate tabulation and referred to as Corporation Research.

The total program, including the Corporation Research, is then carried out in the division laboratories. Periodic reviews of programs are made by the Research Committee as progress is reported, but, by and large, once the programs are formulated, the prosecution is left to the division laboratory managements.

Currently, about one-sixth of our research budget covers Corporation Research, *i.e.*, research of a basic and exploratory nature. These studies of nature and natural phenomena, more often than not, are difficult to associate with immediate business objectives, but we believe that a gratifying number of such projects will ultimately lead to knowledge and discoveries having important commercial consequences.

The sixty-odd projects now in our Corporation Research Budget include such topics as enzymes attacking proteins and insect physiology and toxicology. These are fairly closely related and, in a way, may be said to be associated with a business objective, namely, that of ultimately producing potent insecticides at a profit. We have been screening potential insecticides by empirical means for twenty years with fair success. We now hope, in this instance, that the knowledge we gain by studying chemical

interference with the action of enzymes which control the mechanism of transmission of nerve impulses will enable us to proceed more intelligently.

The Corporation-supported basic research program, especially the scale on which it is being undertaken, is regarded by us as a long-term experiment which is almost certain to succeed. The chief questions remaining are "How well?" and "How soon?"

PATENT POLICIES AND PRACTICES OF INDUSTRIAL RESEARCH INSTITUTE COMPANIES

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The Industrial Research Institute surveyed its 140 member companies in 1957 regarding their patent policies and practices. Replies were received from 104 companies. These 104 companies together had, in 1956, annual sales amounting to some $50 billion. They had research budgets totalling in excess of one billion dollars, representing perhaps 25% of the total spent in industrial and government research in that year. Both large and small companies responded. While 26 of them at that time had over 500 research and development employees, 27 of them had 100 or less. The responding companies would thus seem to be reasonably representative of industrial research and therefore it was thought that the results of the survey would be of interest to readers of *Research Management* as well as to I.R.I. companies. Accordingly, this summary has been prepared. We have tried to present the data without drawing conclusions, leaving the reader to draw his own. The data refer to the calendar year 1956 and are in terms of the definitions used in the 1954 National Science Foundation Survey of Research and Development.

* Other members of the Vital Statistics Committee who assisted in conducting the survey were R. T. Armstrong, L. P. Biefeld, E. X. Hallenberg, A. C. Herro, W. W. McDowell, and R. M. Otis.

Organization of the Patent Function

About one third of the reporting companies indicated that the patent function reported to general legal counsel, about one third to research and development, and about one third to some other authority in the company.

Royalties or other patent income are credited to general corporate income in 75% of the cases and to research and development in only 5% of the cases. Two companies reported that the patent and legal office received such credits.

In contrast, patent litigation expenses were charged to general corporate expense by only 43% of the reporting companies. Thirty-three per cent charged such expenses to the patent and legal office. The remainder reporting charged these expenses to either research and development or some operating division.

The decision to file patent applications was reported to rest solely with the patent function in only some 15% of the companies. The remainder of the companies arrived at this decision by some combination of patent function with research and development or other company divisions through the use of committees.

Some 55% of the reporting companies indicated that the patent function was handled by company patent employees; 10% reported that the patent function was handled by outside counsel; 35% reported that both company employees and outside counsel were used.

Patent Policies

In the matter of patent-assignment agreements, 23% of the reporting companies require them of all personnel, 49% require them of all technical personnel, and 18% require them of all research and development personnel. Ten per cent reported that patent assignment agreements are not used at all.

It is of particular interest that 60% of the reporting companies do not give awards to inventors for specific inventions. Sixteen

per cent give awards of one dollar each. For the remaining 24%, the awards generally fall within the range from two to two hundred dollars, although a few companies give awards of more than two hundred dollars. Thirteen per cent of the companies use special bonus awards in amounts up to five thousand dollars. Two companies use royalty participation as an award.

Nine per cent of the respondents indicated that they did not require a patent review before the initiation of a research and development project. However, 30% always made patent reviews before filing patent applications, and 56% usually made such reviews.

Operating Ratios for the Patent Function

The ratio of research and development personnel to patent personnel increased somewhat with the size of the company, ranging from about 30:1 for companies with less than 100 research and development personnel to about 70:1 for companies with over 1000 research and development personnel.

The average number of patent applications filed per year per patent attorney varied from ten for the smaller companies to fifteen for those with over 500 research and development personnel.

There was no correlation between the number of patent applications filed per year per professional research and development employee and the size of the company, this ratio ranging from 0.4 to 0.8.

Some 46–49% of the formal disclosures acted upon were translated into formal patent applications. The ratio was essentially constant over the period from 1952 to 1956.

Patent Office Experience

There appeared to be a slight trend in the average ratio of patents issued to applications filed. In 1952–53 this ratio was about 65%; it had decreased to about 50% in 1956. Information

obtained from other sources indicated a considerable increase over the last few years in the number of applications filed. Hence, the decrease in the ratio of grants to applications may be due simply to the accumulation of applications still under action.

A large majority of the responding companies indicated a minimum time between the filing of an application and the issuance of a patent of one or two years. Some 60% indicated a maximum time interval in excess of five years. The average time interval was three to four years; it increased slightly from 1952 to 1956 because fewer applications were acted upon in minimum time.

It is of considerable interest that 80% of the respondents felt that the times currently required for Patent Office action did not constitute a considerable handicap in their research and development or business activities.

The Inventors

Only about 10% of the inventors named in patent applications were under 30 years of age. Some 20% of the inventors were 31 to 35 years old, and nearly 75% were 36 years of age or older. The survey responses indicate that, at least with regard to patent applications, invention is not confined to the young scientist.

From 1952 to 1956 there appeared to be a slight trend toward more patent applications with multiple inventors. In 1952 36% of the applications filed were for two or more inventors; in 1956 this had increased to 42%.

Patent Holdings in Perspective

It is interesting to note that 97% of the responding companies indicated that income from patent licenses was less than 5% of total corporate income.

Sixty per cent of the respondents seldom or never decide against doing research and development work because patent protection does not seem to be obtainable.

THE DUAL HIERARCHY IN RESEARCH

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In recent years, industry has become increasingly dependent on technological innovation as an instrument of competition. Within the individual firm, power tends to shift to those who possess the skills most needed for survival and growth. Over the past half-century, this distinction has passed from manufacturing to sales and thence to research and development. Staff-line organization ideology, which developed as a tribute to the pre-eminence of manufacturing, has slowed and obscured shifts of power, has been modified to become a theory of organization applicable to the structure of subunits of the firm, and has tended to give way to functional ideology which allows power to flow more freely in response to changing environmental demands.

The shift of power to research and development has been hampered by staff-line theory which defined it as an advisory service rather than an innovative force or a higher center of intelligence. A more ancient ideology which developed as a tribute to the rise of the bourgeoisie, based on the classical economic theory, also hampered the shift of power by defining monopoly as evil and a short time perspective as good. The innovative process is a delayed response system: research requires that the firm have a cushion of monopoly to support it and survive the period until another cushion can be provided by innovation; thus, the firm must have a long time perspective. The shift of power has been further hampered by the ideology of science

which denies the relevance of social power to scientific work and by the social norms of physical science which exclude and deny the validity of certain types of knowledge and skill required for innovation—political, economic, organizational, social, psychological, cultural, etc.

The relatively weak position of the research and development laboratories of many firms is reflected in their acceptance of the organizational traditions of the rest of the firm, even though these traditions are adapted to the requirements of production rather than research. Until recently, few industrial laboratories have abandoned the organizational model suited for efficient repetition of operations and sought a model more in keeping with the requirements of innovation or the mobilization of intelligence.

It is by no means certain that solutions to problems of coordination, control, evaluation, program formulation, personnel maintenance, decision-making, and the provision of administrative services can be found which do not, of themselves, interfere with the mobilization of intelligence. However, the process which produces these problems also produces a managerial class which is concerned with them and with the mobilization of intelligence. Research managements are experimenting with new organizational arrangements intended to resolve the dilemmas of laboratory organization which confront them.

The problems of research and development organization which confront research managements are not necessarily defined by them in the terms used in this paper. Problem definition is influenced by the situation of the definer. The experience of a research manager is more likely to be organized as follows. As a technical organization grows in size, problems of coordination, control, evaluation, program formulation, personnel maintenance, decision-making, and the provision of administrative services become more complex and burdensome. A managerial class develops which is concerned primarily with these matters. The class is hierarchically differentiated as is the custom in most large-scale organizations. Since its responsibility entails control over the activities of scientists

and engineers, it is logical that technical competence be one criterion for entry into the class. However, entering the managerial class removes the technical man from direct participation in technical work, and he comes to devote himself to many matters not recognized as technical. When a good scientist is made a manager, a good scientist is lost. Yet, promotion to management is the reward for competence in scientific work. Hence, the laboratory becomes a school for making nonscientists of its scientists.

This anomaly having been recognized, the intelligent managerial action is to seek a corrective device, a means of preventing the undesirable outcome. Getting rid of an undesired consequence without, at the same time, destroying the institution that produces it or producing other, equally undesirable consequences in its place, is, however, rarely an easy undertaking. Research managements have experimented with a number of methods and approaches. Two of these will be described and discussed in the following pages.

The Concept of Technical Direction

In simplest terms, the problem appears to be to find a way of rewarding scientists for good scientific performance without removing them from scientific work. One approach to this problem emphasizes the concept of technical direction. For example, administrative assistants, whose duties are to perform nontechnical functions for research supervisors and managers, may be provided to enable the latter to spend their time directing technical activities and thus to continue to make a scientific contribution. At the same time, it is felt that, by having a high degree of technical competence represented in the laboratory management, better results will be assured.

Emphasis on the concept of technical direction produces new problems. Commonly experienced difficulties include the following.

(*1*) *The search for inspirational leaders.* The skills involved in technical leadership are not well understood, nor do they appear to

be readily transferable. Some scientists are gifted in this respect, but the demand appears to exceed the supply.

(*2*) *The productive scientist who is not acceptable to others.* When there is evidence that a scientist lacks the aptitude for technical direction, there is no alternative way to reward him for excellent scientific performance.

(*3*) *The morale of administrative assistants.* If the administrative assistant is a scientist by training, he is likely to regard his administrative post as evidence of failure as a scientist. If he were a successful scientist, he would be a technical director, or at least a practicing research worker. If he is a nonscientist, he is likely to sense disparagement of his skills on the part of the scientific staff. In either case, a satisfying career is difficult to work out.

(*4*) *The effect of a hierarchy of technical directors.* Emphasis on the concept of technical direction implies that technical directors have a responsibility for technical decisions. Inspirational technical leadership is one side of the coin; control is the other. The inspirational leader need not be a controlling figure, but it is difficult to avoid producing a system of authoritarian control where there is a hierarchy of technical directors.

The most serious criticism of emphasis on the concept of technical direction is that it is wasteful of the laboratory's scientific resources. The exercise of technical judgment by a technical director may be regarded as top company management's way of decreasing the likelihood of erroneous technical decisions, since top management must usually accept the technical aspects of laboratory reports on faith. But the technical director's knowledge is inevitably inferior to the combined resources of his scientific staff. Hence, his contribution is likely to be redundant, at best, or negative. The greater his knowledge, the more likely is this danger to be overlooked, and the more likely is his staff to be dependent on his scientific resources rather than on their own. The danger that poor decisions may be made does not derive from the danger that the director may be technically incompetent, but from the danger

that he may not know how to ensure that the scientific intelligence of his staff is brought to bear on problems.

The Dual Hierarchy or Technical Ladder

Some of the difficulties associated with emphasis on the concept of technical direction have led research managements to experiment with another approach to the problem of mobilizing scientific intelligence. This approach is called the "dual hierarchy" or "technical ladder" method. It springs partly from the same problem definition as do the experiments which emphasize the concept of technical direction: namely, that the best scientists are lost when they are rewarded by being made managers. A companion problem, namely, that some able scientists are, in their own opinion or in the opinion of their associates or of management, singularly unfitted for carrying managerial responsibilities, also motivates the dual ladder approach. It is intended to make the practice of research as rewarding or attractive as the practice of management.

The dual ladder approach is roughly as follows: The laboratory, let us assume, is divided into departments, each with a department head. Each department is divided into sections under section heads, and each section is further subdivided into work groups of scientists and engineers, under group heads. This constitutes the managerial ladder. Now, another set of positions is established, roughly paralleling the department head, section head, and group head positions in terms of salary, job luxuries, and freedom of decision on work matters. Terms are invented to label these positions, for example, Scientific Advisor, Senior Research Associate, and Research Associate, in descending order. This constitutes the technical ladder. Research Associates report to Section Heads, Senior Research Associates to Department Heads, and Scientific Advisors to the chief executive of the laboratory. No managerial responsibilities encumber the freedom of persons occupying these positions. This makes it possible to give recognition and reward to scientists who do outstanding work by promoting them in the tech-

nical ladder, at the same time providing them with opportunity to continue in scientific work. The scientist has two ways to advance in the organization.

There are several variations of the above approach, but they share in common the idea of acquiring and maintaining productive scientists in scientific work by rewarding them with prestige, freedom, and job luxuries (special parking spaces, comfortable offices, secretaries, private laboratories, etc.) As with the approach previously discussed, certain difficulties are associated with the dual ladder method. Some common ones include the following.

(*1*) *Role definition for the technical ladder position.* Usually, the keynote of upward movement in the technical ladder is meant to be freedom in research. According to scientific mythology, such freedom is highly valued. In the context of the industrial laboratory, however, it may not be desired by the scientist, for any of several reasons. For some, such freedom is loneliness, rejection, the feeling of not belonging. It is valued highly only by relatively autonomous people. Frequently, the technical ladder position seems appropriate for those members of the staff who are regarded as "prima donnas," that is, for people who are already experiencing rejection by their associates. The reward of a technical ladder position confirms their worst suspicions. A decrease in productivity is sometimes noted after the appointment of a scientist to a technical ladder position; while this may be attributed to his resting on his laurels or to inadequate supervision, it may also be his reaction to discovering that the means he has been using to impress others and gain recognition and acceptance have failed, and that he has been formally isolated from the rest of the staff. In some dual ladder systems, the incumbent is expected to serve as a consultant to others in matters affecting his specialty. The provision of consultant aid requires a high degree of skill, sensitivity to the feelings and needs of others, and ability to help without damaging the other's self-esteem. An appointment to a technical ladder position is, however, rarely accompanied by training in consulting methods; moreover, there is a tendency to appoint scientists who show skill in human relations to

positions in the managerial ladder. Once again, the technical ladder incumbent is likely to experience rejection and to react in ways which are not serviceable to himself or the organization.

(*2*) *The technical ladder position as reward rather than opportunity.* When the emphasis in promoting a scientist to a technical ladder position is reward for past service, certain difficulties may be encountered. Scientific mythology tells us that the scientist is motivated by curiosity. The picture is usually presented of the scientist as one possessed by a burning desire to undertake some favorite projects, from which undertaking he has been prevented by the requirements of his present job. Appointment to a technical ladder position may succeed primarily in revealing the scientist to himself and his associates to be lacking in the independence and commitment which he was formerly free to pretend.

(*3*) *The technical ladder position as a shelf.* The reward aspect of appointment to a technical ladder position, together with its capacity for isolating the individual from the rest of the organization, makes it a convenient shelf for senior staff members whose managerial or technical skills are outmoded or wanting, but who occupy influential positions. To the scientific staff, such appointments are an admission by management that promotion to a technical ladder position is a punishment rather than a reward.

(*4*) *The technical ladder position as an ambiguous status symbol.* Within his profession, there is no universality of title meaning for the industrial scientist as there is for the academic scientist. While ambiguity may occasionally serve purposes of face-saving or making an impression, it is a source of stress when the scientist is trying to identify himself relative to other members of his profession. The problem of identification may be more stressful in the nonscientific community of neighbors and family, where status and worth are identified by widely recognized labels and by such criteria as the size of organization the individual commands.

(*5*) *The technical ladder position as proof of inadequacy.* The scientist who is gifted in his ability to inspire and lead others as well as in his science belongs logically in the managerial ladder. The

technical ladder is usually reserved for competent and brilliant scientists who are regarded as lacking in managerial potential. In our society, leadership skills and leadership positions are highly valued. The technical ladder incumbent is only half a man. Under these circumstances, promotion to a technical ladder position is a dubious honor: it is as much stigma as reward.

(*6*) *Promotion in the technical ladder as mobility up and out.* The characteristics that make the technical ladder a convenient shelf make it unattractive to many scientists. It is peripheral rather than central. It removes the scientist from the main stream of organization. It gives freedom in place of power, but power is needed to remain free. Moreover, as noted above, appointment to a technical ladder position is, in many laboratories, a judgment that the incumbent is unfit to exercise power. This permanent exclusion from the main stream of organization means that the technical ceiling, in terms of income and prestige, is low in comparison with the managerial ceiling. A combination of managerial and technological skill is highly valued in modern corporations. The research scientist or engineer may well aspire to a position in top management if he begins to move up the managerial ladder; but a step up the technical ladder is towards a point of no future and no return.

(*7*) *A shortage of rungs in the technical ladder.* In some laboratories, the growth of a technical ladder has been topsy-like. In these cases, it often happens that there are only one or two prestige and income categories, with the result that a scientific career is out of the reach of scientists who are only competent but not brilliant or, alternatively, that the ceiling for advancement in the technical ladder is so low as to be unattractive to ambitious scientists.

(*8*) *Technical ladder positions are less secure.* The productivity of the scientist in a technical ladder position is more easily assessed, or at least more open for inspection, than the productivity of a manager. Especially if he is doing independent work, where he is relatively isolated from the rest of the staff, there is no difficulty in fixing the responsibility on him if his results do not come up to the expectations of management. In evaluating a supervisor or manager,

there are many considerations and many areas of irreducible uncertainty. Unless the scientist in the technical ladder has been able to take a valued consultant role, however, his situation is uncomfortably simple. Moreover, much more is expected of the scientist in a technical ladder position than performance of the routine tasks that comprise the bulk of scientific work: he is expected to be creative. Persons in the managerial ladder, however, may be highly regarded for providing leadership to groups performing work that is largely routine.

That the dual ladder is a sensitive system, subject to breakdown from many causes, does not demonstrate that it is unworkable, but rather that, to thrive, it requires conditions which are hard to establish and maintain. Some prescriptions and proscriptions can be inferred from the foregoing list of pitfalls.

First, the opportunity rather than the reward aspect of the technical ladder position should be stressed. It should be determined whether the candidate does have a burning desire to do some independent work. This consideration suggests that the technical ladder position should be a temporary, rather than a permanent commitment.

Second, it suggests that research workers who are primarily oriented to success in their profession rather than in the company are the most appropriate candidates for technical ladder positions. If the scientist regards the laboratory simply as a convenient place to do his work, he is more likely to feel liberated than isolated in the technical ladder.

Third, it suggests that training in consulting skills and the provision of consulting opportunities are important integrating elements. The scientist in the technical ladder can still be an influential member of the organization.

Fourth, it suggests that great care should be taken in the selection of candidates for technical ladder positions, since the prestige of the technical ladder rests only on the willingness of good scientists to accept positions in it. Relegation of deadwood to the technical ladder destroys its potential value.

The Problem of Organizational Influence

The most important issue is the problem of organizational influence or power. It is interesting to note that, in the British Scientific Civil Service, the assumption appears to be made that anyone can manage, but that it requires rare talent to do research. There, the equivalent of our technical ladder positions are highly prized, and high scientific rank achieved largely through managerial advances is regarded as less valid than high rank achieved by climbing the technical ladder. In the United States, however, it is doubtful that we can ever support, with enough evidence for it to become reality for scientists, the myth that science is a more important or more valid activity than management. Some laboratories have been successful in removing the stigmata from technical ladder positions by transferring scientists from one ladder to the other in accord with research requirements, research workers' inclinations, and career considerations.

More fundamentally, however, it may be desirable to re-evaluate the functions of members of the managerial hierarchy. Emphasis on control and direction of a professional group seems almost a contradiction in terms. In the professional mythology, a research manager can scarcely accept responsibility for the work of his staff; that responsibility is their own. Professional mythology is, however, no match for the realities of American life. For many young scientists and engineers, the practice of their professions is primarily a means of getting a management position.

The denial of desire for power in the official version of the scientific career is a source of confusion for the scientist, rather than a valid basis for a calling. The young scientist has not acquired the self-image or the skills required for sharing responsibility. Dependency pays off in the laboratory; it makes a cooperative subordinate, suitable for promotion to a position of responsibility. Thus, authority and dependency are the bases of laboratory organization, and a technical ladder must be added to free some scientists for creative work. There is needed a new concept of re-

search management, which helps nominally professional workers to acquire the self-image and skills required before they can accept professional responsibility. Unless scientists are able to participate responsibly in decision-making, to use each other's resources, to engage in mutual evaluation, and to work autonomously, a controlling, centralized management is necessary.